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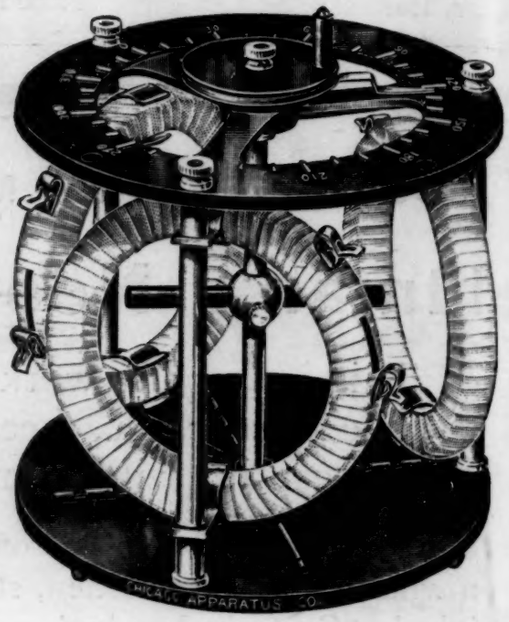

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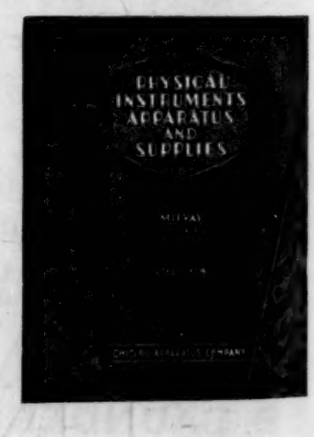

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PROBLEMS OF THE ENGINEER

By Dr. FRANK B. JEWETT

BELL TELEPHONE LABORATORIES

AFTER a great many years of activity in one branch of the engineering profession, and through it having had some considerable contact with practically all the other branches of engineering, it seems to me that we are approaching an era—possibly we are already in that era—when the problems of the engineer, and particularly the problems of the engineering profession, are about to undergo some rather radical changes in their fundamental features. Whether we like it or not, and whether we would change the situation if we could, it seems clear not only that we are living in a highly mechanized age, but also that we and our children and their children are destined to continue to live in an even more highly mechanized environment. Periodically we hear or read statements and wails to the effect that it would be nice if we could return to a simpler mode of living. Any such possibility seems to be quite out of the question, however. The things of science which have been made useful to the people through the work of engineers have not only come to stay but are destined to increase in number. They have come in such large measure already, and they bid fair to come in such greater measure in the years ahead that many of the old controls which were developed through long ages of human activity no longer suffice for a proper ordering of them for the well-being of society.

We have about us everywhere the evidences of attempts to control a new scheme of living through the rules and regulations which grew up in an essentially agricultural and trading age. We are witness to all sorts of legislative action taken in an endeavor to control by statute things which can not be con-

[1] Address of the retiring vice-president of Section M—Engineering, American Association for the Advancement of Science, New Orleans, Louisiana, December 30, 1931.

trolled except in the light of a complete understanding of their scientific and engineering significance. Under the circumstances in which we find ourselves, it seems to me that if the people of the world are to live happily in the years ahead, and if their affairs are to be ordered in measurably decent fashion, a better general understanding of the things of science and engineering is imperative. Nor can this better understanding be confined to the limited few who make up the group of scientists and engineers. It must be an understanding which in some measure enters into our collective thinking and into our acts as a group of human beings bound together by what we commonly call political ties. If this premise is correct it would appear that the engineer must of necessity play an increasingly important part in the general scheme of things, since he is the only man who in general has that full understanding of the facts of science as applied to the affairs of everyday life which is necessary for a proper operation of the new controls.

As I look back to my own experience, and still farther back through history, particularly the history of these last one hundred years, it seems evident that we of the engineering profession have been in principal measure so largely concerned with and so interested in physical things, in the creating of new structures and of new applications of old and new science, that we have been content to let the social significance of our work rest wholly in the hands of non-scientific or non-technically trained people. It is true that, individually, in practically every activity in which we have engaged, we have had some influence in molding affairs. Nevertheless, acting as a group, we have not taken the same part in the evolution of society in this mechanized age that I think we are to and must take in the years ahead of us. If you grant this premise, then it seems to me obvious that there are two main problems—three main problems possibly—to which some thought must be given.

In the first place there are the problems of education for the young men who are to be our engineering successors. Up to the present time our scheme of engineering education has been essentially one which acquainted young men with the fundamentals of their profession and something of the groundwork of science, on which all engineering is based. It has been concerned in addition with the rules by which science is applied in the profession of engineering—rules which in the last analysis are nothing but the job of mixing fundamental science with dollars in an economic structure. Of course this aspect of engineering education is bound always to be a very large part in any scheme of technical training. There must always be a large number of men who are competent to take the facts of science and the rules of engineering and apply them for practical purposes. Further, no man is likely to achieve to real eminence in the broader aspects of a social service based on engineering unless he is himself competent to do and judge some at least of the things which are the hallmark of good professional engineering.

Beyond this basic requirement, if I am right in my picture of the years ahead, it may well be that in some at least of our engineering schools—possibly in most of them—more emphasis will come to be laid on the social side of the engineering profession. Where this is done the objective will be so to endow the young men who go out from the schools that they will be competent not only to do the kinds of things that you and I have been called upon to do in our active life, but more competent than we to take their places in the whole scheme of evolving society.

Then there are the problems which affect us in our professional association one with another, that is, in our technical societies. In the past our societies have been in large measure places of meeting where technicians could gather together for a general interchange of ideas, for a discussion of technical problems and for the taking of whatever group action seemed advantageous for the advancement of our profession. Possibly in the years ahead our engineering societies, either through modification of their present organizations or in their associations one with another in such things as the American Engineering Council, will find it expedient and necessary to equip themselves in better fashion to play the part which society has a right to demand of us, its experts in this particular field, in bringing to play on the problems which will confront our people the general consensus of understanding which we alone possess in the greatest measure.

Finally, there are the problems of a better education of the great rank and file of the population who have an interest in but no specific concern with the practical applications of science. I surmise that a large part of the things which we are prone to criticize in the acts of our political leaders, our political parties and our various social groups, are not so much due to any malicious desire to warp the situation as they are due to a fundamental ignorance of the factors which are involved. In a word, where these acts seem to play ducks and drakes with the facts of science and engineering they are, I think, in the main, the unintentional result of insufficient understanding.

We who are engaged in any field of science or its application know that the most dangerous thing we can do is to fool ourselves, or permit ourselves to be fooled, with regard to the facts of science or engi-

neering. Old Dame Nature has a very unpleasant way of never fooling herself or allowing her votaries to be fooled with impunity. If we engineers either consciously or unconsciously misuse or misapply facts, we are pretty sure sooner or later to find ourselves in serious difficulties. For the engineer who finds himself in this kind of trouble there is in the main no extenuating alibi. This being so, and if I am right in my suspicion that a large part of the things we now criticize is due to ignorance on the part of the public generally, then, to such an extent as is possible with a great mass of human beings, we owe it to ourselves and to the society of which we are a part, to do what we can to bring about a better general understanding of the possibilities and limitations imposed by nature. In a word, where the facts of science and their application are really the controlling factors in any proposed legislative and administrative undertaking, general knowledge of the inherent possibilities and limitations is highly advantageous.

All of us have had numerous illustrations indicative of the abysmal ignorance of men and women of intelligence about things which seem to us the everyday commonplaces of life. We have equally, I think, all had the experience of seeing how easy it is to educate intelligent individuals or small groups of individuals, if we can only get them to listen to what we have to say. Educating the occasional individual or occasional small groups, as to some particular topic, is, however, quite a different problem from that of educating millions of people. Nevertheless, it seems to me that these millions of people must, in the years ahead, come to have a better understanding of the fundamentals of science and engineering than have their parents and grandparents. Moreover, we scientists and engineers must in some way or other take a real part in bringing about that education. Further, either individually or collectively, we must be prepared to play a real part not alone in our purely technical activities but as citizens of the community and of the nation in which we live.

So much for some of the reasons which lead me to believe that we are on the verge of a different kind of engineering life than we have experienced these last forty or fifty years. Now there are two things which seem to me to indicate a change in the point of view and, through it, in the activities of engineers as regards the more strictly professional side of their work.

One of these is that in many fields of engineering we seem to be reaching the ultimate of possibility in our applications of the things of science. I use the word "seem" advisedly, for frequently when I have thought that we were clearly at or near the end of our rope I have had brought to my attention something which caused me to wonder whether my conclusions were as sound as I thought they were.

Take, for example, the matter of dams. While I am not a civil engineer, I do know a little of civil engineering and am cognizant of the fact that dams to impound water are among the most ancient of man's engineering structures. I presume that the first dams were built untold centuries before there was any recorded history. This being so, one might easily think that in all the long period of human history men would have had experience with every conceivable element entering into the construction of dams, and so would know all there was to be known about them. Yet when one talks with one's civil engineering friends, and when one reads the more or less acrimonious discussion which goes on with regard to the stability of gravity dams, the cause of failure of such things as the St. Francis dam, and the probable stability of the great new Hoover dam, one begins to wonder just how much we really do know about them.

Despite such uncertainties there are nevertheless some fields where there seem to be fairly clear indications that our prsent applications have reached close to the limit of the possibilities permitted to us by the fundamental facts of science.

As an example, I can cite one illustration in my own field of engineering where we seem close to the limit. Not being clairvoyant, I am even here, however, uncertain as to what future science may have in store for us. What I do know is that in the present state of scientific knowledge we in the communication field have in some directions just about reached the point where we can not hope to go farther along lines which have been tremendously fruitful in the past. As an illustration of what I have in mind, let me cite a simple case involving the transmission to great distances, of electrical impulses. Fifteen years or so ago very great advances were made in extending the range of electrical communication through the development and introduction of distortionless amplifying devices. You are all acquainted with one form of such devices in the vacuum tubes of your household radio sets. As employed in the extension of electrical communication these devices are made to control relatively large amounts of fresh energy supplied to the transmission channel at points distant from the sending station, in such a way that the minute control impulses which reach them are magnified many fold without distortion of form. In a word, they are employed in such manner that they appear to a still more distant point as the relatively near-by primary origin of the impulses.

These devices of practical utility, which grew out

of fundamental research in physics, opened the door to what appeared to be an almost illimitable amplification of very small amounts of energy without distortion. Initially it appeared as though one might go on practically ad infinitum in the multiplication of energy under the control of very weak impulses and so provide for spanning almost any terrestrial distance which might be involved in electrical communication. For a considerable number of years after their initial introduction this assumption seemed to be quite within the bounds of practical possibility. It still appears to be within those bounds wherever it is reasonably possible to provide intermediate amplifying equipment to the communication channel at suitable intervals.

Recently, however, in our attempts to span very great distances where we were deprived of the possibility of providing these revivifying agencies, as we can on land lines, we have come face to face with what appears to be the stone wall of a fundamental obstacle inherent in the nature of matter. Specifically, we have encountered this obstacle in our attempts to bridge great ocean distances, such as the Atlantic, with a telephone table. Here, because we are deprived of the possibility of installing intermediate amplifiers, we must, if the problem is to be solved, at all, depend, at the distant end of our circuit, on the minute remnant which remains of the energy imparted to that circuit at the sending end. This might not be a serious obstacle if we had to deal only with the fact that this remnant of energy was minute. All that might be required in such a case would be the introduction of a train of amplifying devices each adding its increment to the elevation of the energy level.

What we do encounter is, however, something quite different. We find the remnant of energy with which we have to deal is so minute that it is approaching the level of the electrical energies involved in the noises produced by thermal agitation within the material of the conductor itself. A short step further in this diminution, such as might easily be produced by an extension of distance, will bring us to the point where no amount of amplification can be of service, since any amplification will elevate equally both the desired impulses and their masking background of noise, and we know of no way of separating the two. Unless and until, therefore, some one finds either a metal which has a lower background of inherent noise, or a way by which that background of noise, which covers the entire spectrum, can be gotten rid of, there is a definite limit to what we can do.

Other examples might, I think, be cited to indicate that in certain other branches of engineering there are in the present state of our knowledge definite limits to what we can do in an engineering way. Where such fundamental obstacles are found to exist it is obvious that the past course of engineering development, on its more technical side, must be altered. In a word, we will have in such fields reached the frontiers and must consequently direct our energies to developments within those frontiers. Put another way, it means that in those particular sectors the engineering of the future must be directed to the problem of filling in gaps which now remain, in bettering the structures which we have already erected inside the frontier, in cheapening these structures, or in the thousand and one ways with which we settle up an already discovered country.

In other directions and in other branches of engineering it is quite obvious that we have not yet reached the point where such limits have begun to operate. In these fields progress can still be made along the old lines, although even in such areas I imagine that, relatively, we shall see much more attention given to the filling-in processes as we go along than may have been the case in times past.

There is another direction in which it seems to me that the engineering profession of the years ahead is likely to have a somewhat different point of view and a somewhat different method of approach from that which we have known in the past. Fundamental science has been bringing to us as raw material for our profession, these last three or four decades, a perfectly stupendous amount of new knowledge. New facts thoroughly proven in the research laboratory have been placed at our disposal to be applied as were the older facts which were their predecessors. In the main these new facts have had to do primarily with the ultimate structure of matter. They have placed and are placing in our hands smaller building bricks than we have had to deal with, and very much smaller building bricks than our engineering predecessors of a few decades ago had to deal with. All of us know that finer and more substantial structures can be made with finer ultimate elements than can be fashioned out of cruder things. To the extent therefore that this is so, it means both that our engineering structures of the future and the engineering attack which produces them must be concerned more with intimate details than has been the case in the past.

Further, in many branches of engineering, this added knowledge which the physicists and chemists are placing at our disposal is indicating quite clearly the basic causes of some of those failures which in the past we have guarded against by the rather crude processes of enlarged dimensions which we have come to designate as factors of safety. To me these so-called factors of safety should in fact, to a large

extent, more properly be designated as factors of ignorance. Factors of safety will still be involved to insure against the unknown in the matter of maximum stresses, but they should in large measure be eliminated insofar as the inherent properties of the structural material itself is concerned.

Obviously, the more we can know with certainty about the things with which we deal, the farther we can go in refining our engineering in the design of our structures. This fact alone seems to be certain to have a very considerable influence on the type of education which we shall afford the young men who are to be the engineers of the future. Those among these young men who have in them the ability to reach the top of their profession, in whatever field of engineering, must be provided with a better knowledge of physics and chemistry and the underlying principles which guide their practical application than you and I were given when we were going through the rudiments of our technical education.

As an illustration of the necessity for this more intimate knowledge of basic things, it is easy to show in some branches of engineering that such knowledge is absolutely essential to progress and safety. For example, we have in recent years learned that in certain alloys and aggregates very minute admixtures of certain things produce effects which are out of all proportion to the amount of the extraneous material. Frequently these effects have been deleterious and the minute admixtures have been referred to as poisons. In other cases the effects have been beneficial.

Again, turning to the field with which I am best acquainted, I can cite you an example of what I have in mind.

Some years ago, in our quest for a magnetic material which would be more magnetic than the best irons and steels which were then available, and which, if produced, would open up wide avenues of progress, we evolved an alloy of nickel and iron. As I remember it, it was an alloy containing about 78 per cent. nickel and 22 per cent. iron. This alloy, when properly fabricated and properly heat-treated, had very remarkable magnetic properties. For the magnetizing forces with which we have to deal in communication circuits it was found to be many times more magnetic than the best irons and steels previously available and to have other desirable properties. It was consequently a very valuable addition to our store of structural materials. At the same time we learned, however, that very small admixtures of certain ingredients into the alloy had tremendous deleterious or poisoning effects on the magnetic properties of the alloy.

This discovery was a striking illustration not only of the effect of small things in upsetting the calculations which might otherwise have been made, but also of the dangers which one runs in proceeding too boldly to use some new discovery without the most complete and exhaustive investigation of the things which may result from slight deviations from absolute perfection. If I remember correctly, some rather disastrous consequences have followed attempts to use some of the newer steels or some of the newer forms of heat treatment of steel in mechanical structures. In all such cases which come to mind, the difficulty was either lack of complete understanding or failure to enforce rigorously the known controls in the factory.

Thus in many directions the engineer of the future, in my judgment, must of necessity deal with a much more certain and more intimate knowledge of the materials with which he works than we have been wont to deal with in the past. As a result of this more intimate knowledge his structures will be more refined and his factors of safety in many directions are bound to be less because the old elements of uncertainty will have in large measure disappeared. All in all he will be working on a sounder base of knowledge than has heretofore been possible. To the extent that this is possible, it must inevitably have a very considerable influence on the type of education given in our technical schools, at least to the men who have the inherent capacity to develop into the leaders of the engineering profession.

I would add just one word more about the changes which it seems to me are likely to come about in the relation of our successors to our profession. Whatever changes there may be in the intellectual or social phases of engineering, one can not escape a realization of the tremendous interest which resides in the purely physical. It is a type of interest which does not exist in any purely intellectual type of achievement. As one advances in years and experience the normal tendency of the engineer is to become more and more involved in phases of engineering which are essentially intellectual, and to a large extent lacking in personal contact with the purely physical. No matter how much satisfaction one may derive from the successful completion of a task of this kind, it is devoid of much of the thrill that accompanies the creation of a physical thing with one's own hands or through one's own efforts. I know this from my own experience, and while I derive a large amount of pleasure from the solution of the problems with which I now deal, they do not linger vividly in my memory as do the problems of my earlier years. As a matter of fact, I doubt if I derive as much personal enjoyment from the things of my business life as I do from the simple little structures which I make in my own shop merely as a matter of personal relaxation.

There is something stimulating in the creation of things which exist physically, which you can look at or feel, which does not exist in any purely intellectual achievement. This being so, I would not for a moment desire that the training of engineers for the future should so over-emphasize the purely intellectual side of our profession as to blot out the training which will enable men to produce things physically.

We must always have in mind that our engineers, in the last analysis, must be the creators of physical things if they are to be real producers, have a real understanding of their profession, and be able to play the enlarged part which I foresee to be the province of our successors.

As a telephone engineer I get a tremendous kick, whenever I go across the continent, merely in looking out of the car window and seeing such a simple thing as the pole line which carries the transcontinental circuits. Why? Because I realize that some of the best and happiest years of my life were spent in the creation of that very physical thing. At the present time I deal with similar problems almost wholly in an abstract way. I decide what sort of things should be done and I pass judgment on the work of others, but in the main I have no part in the construction of the things themselves. My life is given up almost entirely to paper-shuffling and various forms of intellectual gymnastics, which leave in me relatively little satisfaction.

ON POSTULATES OF PROOF IN PROBLEMS OF THE BACTERIAL LIFE CYCLE

By Professor HANS ZINSSER, M.D.

DEPARTMENT OF BACTERIOLOGY AND IMMUNOLOGY, HARVARD MEDICAL SCHOOL

INTO many of the etiological questions of modern bacteriology there have been introduced problems of far-reaching biological consequences, which must be approached with the grim logic of the bacteriological forefathers unless we are to flounder in a bewildering confusion. These are the matters of the filterable stages of true bacteria and the suggested cyclic relationship between bacteria and the ultramicroscopic viruses. These two questions have often been confused with each other and there is a growing school of investigators from whose work it is often unclear whether they advocate one of these theories only or whether, in claiming the truth of one, they mean to imply that of the other as well. In regard to these developments, bacteriology is in much the same confusion with which it was threatened in the early etiological era, when countless false leads were laboriously followed out by investigators who attached etiological conclusions to the mere isolation of any organism from an animal or man suffering from a specific disease. And the formulation of his postulates by Koch was the warning of an uncompromising disciple of the truth against the dangers of allowing faulty reasoning to leap across experimental chasms. It is time for us to set up, in consultation, similar postulates in regard to the problems alluded to, if we are to avoid the obstructions to permanent progress which loose reasoning always produces. For there is no room for two schools of theory in any of these matters. Either a proposition is demonstrable by experiment, and confirmable, or it is not. And it can only delay the successful pursuit of the truth to claim, as established fact, conditions which have not been so established by observation. No one would be more delighted than the writer of these notes if most of the claims that have recently tended to establish a traceable cyclic relationship between invisible and visible forms of bacterial life could be substantiated. Such an achievement would enormously increase the capacity of bacteriologic methods to clarify important biological and medical problems. But we gain nothing in this direction unless we set down clearly the criteria of proof which alone can justify us in incorporating these new conceptions into the premises of our science. Regarding such criteria opinions may differ. It is with the purpose of clarifying this issue that these notes are written.

Let us consider first the question of the existence of filterable forms of true bacteria. The impulse for the present activity in this field was the great progress made in the knowledge of bacterial dissociation. And, in regard to this, it is gratifying to realize that the horizon of bacteriology and immunology has been infinitely enlarged by the studies of the so-called "mutations" in which cultural and colony studies and their immunological and chemical correlations constitute a series of discoveries achieved by the most rigid observance of that type of self-criticism in experiment which, it seems to us, is often lacking in the fields of inquiry which are the subject of this discussion. That all methods of bacterial filtration are complicated by a host of experimental irregularities is well known. And that there is no satisfactory method of appraising any of the ordinary

laboratory filters except by actual test is acknowledged by every one who has worked with these methods to any extent. In our own work, we have found that neither the measurement of the amount of freshly distilled water which will pass through a filter, under a stated negative pressure, in a given time, nor the measurement of the pressure which will push air through the filter under water, possess any reliable relationship to the permeability of the filter for bacteria. Also, Ward and Tang in our laboratory showed, some years ago, that the passage of a virus through a filter depended as much upon the suspension fluid as it did on other factors and that the agents of vaccinia and of herpes would pass through a filter with far greater ease when suspended in certain types of broth than when taken up in any of the various isotonic salt solutions, irrespective of pH. Grinnell, then, testing all our Berkfeld and Mandler filters with *B. prodigiosus,* showed that all the "V" candles and most of the "N" ones would let these organisms through under relatively low pressure (10 cm Hg) provided the bacteria were taken from old cultures and were suspended, for filtration, in a hormone broth at about pH 7.8. Moreover, to be sure of the "tightness" of filters so tested, we found, in connection with our early typhus work, that control cultures of filtrates could not be regarded as sterile until they had been incubated for at least two weeks, since many of our controls showed the first prodigiosus growth as late as twelve and thirteen days after filtrate inoculation. From these facts alone it is apparent that the mere passage of bacteria through a filter—even a Berkfeld "N"—is not proof of a filterable form.

It is, of course, quite likely that bacteria, which multiply by fission, may well degenerate into granular fragments far smaller than the characteristic species-forms. It is not only conceivable but even likely that such fragments, containing chromatic materials, may be viable and grow out eventually to the adult stages.[1] But this does not constitute a filterable phase of a cycle. It is biologically far less significant and modifies in no respect our fundamental conception of bacteria.

In order to truly demonstrate the principle of a cycle between filterable and non-filterable—or, more exactly, large and minute forms of a morphological cycle, we would set up the following postulates:

(1) It must be possible to filter a suspension of a well-defined pure culture of bacteria through a filter which holds back the characteristic forms; and these original forms should not appear in the filtrate or in cultures made with adequate amounts of the filtrate on suitable media after incubation of at least two or three weeks.

(2) There must be evidence of growth of some kind in cultures made of this filtrate, but without evidence of the presence of the original normal forms.

(3) The culture of the filterable or minute form must be carried in a series of several generations.

(4) It must be possible to recover the normal forms from such successive "filterable attributes.

Such requirements have not, as far as we know, been fulfilled by Mellon, by Rosenow or by Kendall, who are among the chief protagonists of the existence of filterable stages. They have been reported, we believe, only by Hadley, who gives details in regard to his Shiga bacillus filtration, but this work has not yet been sufficiently confirmed. Hadley, incidentally, is also quite definite in rejecting any implication that he suggests identification of his "filterable bacterial forms" with the so-called "ultramicroscopic virus" groups.

When we come to the suggestion that infectious agents, such as those of herpes, poliomyelitis, etc., are merely filterable or ultramicroscopic forms of well-known bacteria, we are confronted with a problem that threatens still greater dangers of possible confusion. It is of course, an obvious thought that has come to every one who has been frustrated by the difficulties which confront those who are laboring for a break-through in this field of investigation. It is not originality, but rather boldness—we might say intellectual recklessness—which seems to us to characterize the claims for such a cycle on anything less than convincing evidence. It is a fascinating way out of our difficulties, and the voice of caution is too apt, just now, to be suspected of unimaginatively academic hostility to new ideas, which is justly regarded as the opposite pole of obstruction to progress.

The claim of such cycles has been made, either in statement or implication, by a number of workers who need not be named, both in connection with herpes encephalitis, poliomyelitis and fox encephalitis. And the point of view has become widely disseminated among clinicians. To those who work with the ultramicroscopic viruses, however, a number of attributes of these agents are obvious which caution them to demand complete proof before they permit themselves to regard them merely as stages in a cyclic metamorphosis of bacteria. These properties, apart from

[1] Every one who has worked with the cholera spirillum and many other similar organisms knows that old cultures show almost no characteristic individual forms. Smears from such cultures are swollen, granular and unrecognizable as true spirilla. Yet, on transfer, the typical morphology reappears. We were, at one time, tempted to interpret, as possible evidence of a new type of bacterial life, the fact that our filtrates of typhus material, controlled with prodigiosus, showed no growth in cultures for as long as twelve days, after which period the original forms appeared in the tubes. This is, however, much less sensationally explicable on the basis of latency of a few organisms under conditions of the unusual environment furnished by filtrates containing many fresh tissue constituents.

the usually ultramicroscopic magnitudes which determine filterability, are the following:

(1) Except for the as yet insufficiently confirmed experiments of Eagles with tissue extracts, no filterable virus has been cultivated except in the presence of living cells in tissue culture.[2]

(2) Many of the viruses have been serially cultivated in tissue culture without reversion to the bacterial form.

(3) They produce diseases which are specific and unlike those produced by any known bacteria, even those produced by the bacteria supposed to have been developed out of the viruses themselves.

(4) The pathological histology characteristic of the virus diseases is entirely unlike that of any of the true bacterial lesions but, on the other hand, there is much similarity between the histological strategies aroused in animal tissues against the various filterable agents.

(5) In the lesions of most (almost all) filterable virus diseases characteristic "inclusion bodies" are present.

(6) There are many indications which render it likely that the theater of conflict in the virus diseases is an intracellular one.

(7) Active immunization in the virus disease is (with two exceptions which are not yet clearly understood) impossible unless living virus is used.

(8) The protective bodies differ from bacterial antibodies both in their limited concentration, even with hyperimmunization, and in the difficulty and uncertainty of demonstrating any *in vitro* reactions.

(9) There is a striking concentration of virus in specific tissues like nervous system and skin.

(10) There is a likelihood of persistence of the viruses in the animal body as long as immunity persists.

In addition to these points there are many minor facts, such as resistance to glycerine, to chemical agents and to heat; relationship to oxidation and reduction and to salt concentrations such as those recently demonstrated by Clark, all of which demand more study—but which, taken together, should caution us against any rash assumptions of relationship to bacteria, however attractive and startling such speculation may appear to be.

We do not want to be understood as throwing out of court the theory of cyclic relationship between bacteria and the virus group. Indeed, the possibility of such a relationship is perhaps the most important problem confronting the bacteriologist to-day. However, there is little sense and no utility in piling up our literature with repeated assertions of the existence of such a condition, unless definite postulates of proof can be fulfilled; and such postulates we would set down as follows:

Starting with a virus capable of producing a characteristic disease with all the clinical, pathological and immunological criteria necessary for its recognition, the investigator must:

(1) Repeatedly cultivate a bacterium of a single well-defined species.

(2) He must carry this bacterium through a number of culture generations. (If he can produce the disease, in the animal susceptible to the original virus and in a characteristic way, with these cultures, all the better. But this need not be required since virulence may change with form. However, we would attach no importance to such experiments as those of Rosenow with poliomyelitis, in which the bacterium produced a disease in rabbits and ginea-pigs—species entirely resistant to the original virus.)

(3) After a number of bacterial culture generations, it must be possible to get back the virus itself—apart from the bacteria—either by culture or by animal inoculation; the culture generations must be sufficient in number to insure complete removal of the original virus by adequate dilution; and such recaptured virus must possess all the original characteristics as to filterability, specific pathogenicity, etc., of the original material.

Such demands have not, to our knowledge, been fulfilled by any of the investigators who have been protagonists of the cyclic idea. Until such postulates or similar ones—adjusted to new knowledge—are required, however, we will get no farther, and this phase of bacteriology will remain sterile. We might add to all this the modest requirement that the experiments reported in such cases should be repeatable by other qualified investigators.

We sincerely hope that our point of view will be vigorously criticized by those who hold these cyclic metamorphoses to be demonstrable facts. The questions involved are of such fundamental importance that they call for the cooperative efforts of serious bacteriologists to remove them from the realm of speculative reasoning and place them upon a basis of experimental proof comparable to that which has been demanded for every other great bacteriological discovery.

[2] We believe that we are correct in assuming that neither the cultivation of the so-called "globoid bodies" in poliomyelitis—nor the cultivation of the agent of mosaic disease are regarded as accomplished facts any longer—even by their original proponents.

SCIENTIFIC EVENTS

PLAN FOR A CHEMICAL CENTER IN CHICAGO

ON January 30, 1932, representatives of the Chicago Section of the American Chemical Society and of the Chicago Chemists Club signed agreements by which both groups will in the future hold their meetings in the Midland Club. In addition, the Chicago Chemists Club signed a lease for quarters considerably larger than they have previously occupied in the Midland Building, 176 West Adams Street, in which the Midland Club is located. The signing of these

agreements marks the termination of negotiations which have been going on for several years to secure enlarged facilities for chemical activities in Chicago. The rapidly growing attendance at meetings of the Chicago Section, which reached six hundred at a special petroleum meeting in January, was making imperative a change in meeting place. The new agreements run for five years and are renewable at the end of that time. In addition to providing accommodations for all meetings of the two organizations, the plan involves the cooperation of the chemical groups in securing tenants from the chemical industry. If this plan is successful and the building secures a considerable number of tenants who are connected with the chemical activities, the name of the building will be changed to indicate its position as a center of chemistry in Chicago.

The Midland Building is said to be one of the newest and finest of the large club buildings in Chicago and is situated in the heart of the business district. It is fully adequate to accommodate the chemical organizations of Chicago. Negotiations were carried out under the leadership of a building committee headed by Dr. Paul Van Cleef. The chairman of the Chicago Section is Dr. Bernard E. Schaar, and the president of the Chicago Chemists Club is Dr. Ernest H. Volwiler.

THE ALLEGANY SCHOOL OF NATURAL HISTORY

THE sixth season of the Allegany School of Natural History in Allegany State Park opens on July 5 and closes on August 24. This "Summer School in the Forest" is conducted by the Buffalo Society of Natural Sciences in cooperation with the New York State Museum and is affiliated with the University of Buffalo from which its students receive college credit. Registration should be made with Mr. Harold T. Clement, Curator of Education at the Buffalo Museum of Science, or with Dr. R. E. Coker, director of the Allegany School of Natural History, Box 950, Chapel Hill, North Carolina.

Courses will be given in field zoology by Robert E. Coker (Johns Hopkins), professor of zoology, University of North Carolina; in field geology by Mr. Frederick T. Thwaites, lecturer in geology at the University of Wisconsin; in field botany by Mr. Robert B. Gordon, Ph.D. (Ohio State), instructor in botany at the Ohio State University; in the natural history of birds by Aretas A. Saunders, Ph.B. (Yale), teacher of biology, Central High School, Bridgeport, Connecticut, and in nature study by Mr. William P. Alexander, B.Sc. (Cornell), field naturalist and assistant curator of education at the Buffalo Museum of Science.

The Allegany School of Natural History is nine miles from Quaker Bridge, New York, and well above it, being located on a hillside bordering Quaker Run in its upper part at an altitude of about 1,900 feet. It is a feature of the setting of the School in Allegany State Park that within an area of some 65,000 acres under the care of the state much of the wild life is protected, and so one may occasionally see bear and deer, besides observing daily the abundant smaller mammals, chipmunks, field mice and, less frequently, jumping mice, shrews, weasels, mink and others.

Teachers in public schools and colleges, particularly those who have had little opportunity for field studies, university and college students, scout and camp leaders of various kinds, young and amateur naturalists, and those interested in the nature work of museums, public forests and parks, are invited to attend the Allegany School.

APPROPRIATIONS FOR GRANTS-IN-AID BY THE NATIONAL RESEARCH COUNCIL

AT its meeting in February the National Research Council's Committee on Grants-in-Aid made twenty-four grants for the support of research as follows:

Carl E. Howe, associate professor of physics, Oberlin College, for the measurement of wave-lengths of x-rays; Jakob Kunz, professor of theoretical physics, and J. T. Tykociner, research professor of electrical engineering, University of Illinois, for studies of the photoelectric effects of alkali vapor and films and a velocity selector for molecular rays.

W. C. Austin, professor of physiological chemistry, Loyola University School of Medicine, Chicago, for investigations on the transformation of aribose to ribose; Walter L. Badger, professor of chemical engineering, University of Michigan, for investigations on the effect of viscosity on the heat transfer coefficients between metals and boiling liquids; Harold Hibbert, professor of industrial and cellulose chemistry, McGill University, Montreal, for research on plant synthesis and immunology; I. M. Kolthoff, professor of analytical chemistry, University of Minnesota, for investigations on the internal structural changes taking place in a freshly prepared crystalline precipitate on standing; Charles P. Smyth, associate professor of chemistry, Princeton University, for research on the dielectric constants of gases.

Ernst Cloos, department of geology, the Johns Hopkins University, for a survey of the Sierra Nevada batholith; Robert S. Platt, associate professor of geography, University of Chicago, for part of the expense of a study of types of rural land occupancy in South America; H. B. Stenzel, assistant professor of geology, Agricultural and Mechanical College of Texas, for field work on the paleontology and stratigraphy of the lower Claiborne formations.

Alfred Chanutin, professor of biochemistry, University of Virginia, for research on the effect of diet in the

partially nephrectomized rat; David M. Greenberg, associate professor of biochemistry, University of California, for research on the biochemistry of magnesium; H. Keffer Hartline, fellow in medical physics, Johnson Foundation, University of Pennsylvania, for a study of visual phenomena as indicated in the impulses in single optic nerve fibers; Carl C. Speidel, professor of anatomy, University of Virginia, for investigations on the fundamental activities of nerves in the living animal.

E. A. Andrews, professor emeritus of zoology, the Johns Hopkins University, for field observations on the variation of snails (*Neritina*) in salt-water ponds in Jamaica; Walter F. Loehwing, professor of botany, State University of Iowa, for a study of the effects of inorganic nutrients on the vegetative and reproductive cycle of plants; William Patten, professor emeritus of zoology, Dartmouth College, for field expenses in collecting material for research on the origin of vertebrates; George Harrison Shull, professor of botany and genetics, Princeton University, for part of the expense of investigations on the genetics of the evening primrose.

Edward F. Castetter, professor of biology, University of New Mexico, for a study of the ethno-biology of the Indians of the southwestern United States; Walter B. Jones, director, Alabama Museum of Natural History, for investigations on the Moundville culture of Alabama; Arthur Randolph Kelly, assistant professor of anthropology, University of Illinois, for a study of the physical anthropology of the Bronze Age people of Greece.

Harold C. Bingham, Sanbornton, New Hampshire, for a comparative study of the psychobiology of gorillas and other anthropoid apes; G. LaVerne Freeman, instructor and research associate in psychology, Northwestern University, for use in a study of diurnal rhythm, including both sleep and waking activity; Donald McLean Purdy, associate professor of psychology, University of Kansas, for the study of an unusual case of defective vision.

GEORGE K. BURGESS,
Chairman

THE WASHINGTON AWARD

THE Washington Award for 1932, "in recognition of devoted, unselfish, and preeminent service in advancing human progress," was presented to Dr. William David Coolidge, associate director of research in the laboratory of the General Electric Company at Schenectady, following a dinner and reception on February 24 at the Hotel Sherman, Chicago. Dr. Coolidge is the eleventh to be so honored by the Washington Award Commission, the first presentation being to President Hoover, in 1919. Dr. Harry Woodburn Chase, president of the University of Illinois, delivered the principal address of the evening, evaluating the importance of research in the present social order.

Among the best-known contributions of Dr. Coolidge are ductile tungsten for lamp filaments, wrought tungsten for contacts and x-ray targets, the hot cathode x-ray tube, various types of x-ray generating equipment, the "C" tube for submarine detection and signalling, and the cathode ray tube.

His achievement of drawn tungsten filaments made the incandescent lamp cheap and reliable, and his improvements in the x-ray tube augmented and made less dangerous to operate that modern laboratory instrument. The beam of Dr. Coolidge's cathode-ray tube, having the power of a ton of radium, has unknown possibilities in the realm of science and medicine. An exhibit of the achievements for which the award was given to Dr. Coolidge was on display at the hotel.

Dr. Coolidge graduated from the Massachusetts Institute of Technology in 1896, afterwards receiving his Ph.D. from the University of Leipzig. Returning to the United States, he joined the teaching staff of the institute, serving as assistant in physics, instructor in physical chemistry, and as assistant professor of physico-chemical research. In 1905 he became a member of the staff of the General Electric Research Laboratory, under Dr. Willis R. Whitney, and in 1908 was made assistant director of that organization, a position he occupied until 1928, when he was made associate director.

The Washington Award, presented to Dr. Coolidge, was founded sixteen years ago by John Watson Alvord, and it is administered by the Western Society of Engineers. Presentation was made by Mr. Frank D. Chase, president of the society.

AWARD OF THE POPULAR SCIENCE MONTHLY

DR. IRVING LANGMUIR, associate director of the research laboratory of the General Electric Company, at Schenectady, N. Y., has received the *Popular Science Monthly* annual award of $10,000 for notable scientific achievement. The award, together with a gold medal commemorating it, was presented to Dr. Langmuir on February 29 at the Engineers Club in New York City.

This award was established two years ago for the double purpose of honoring Americans who have done notable scientific work and of stimulating the public mind to a greater appreciation of the values of scientific investigation. The first winners were Dr. George H. Whipple, of the University of Rochester, and Dr. George R. Minot, of Harvard University, who received the award jointly last year for their discovery and development of a cure for pernicious anemia.

The committee of award consisted of twenty-two leading men of science, and nearly 1,800 organizations were requested to present nominations.

Dr. Langmuir is known for his contributions to the knowledge of atomic structure; the theory of the single molecular layer; the heat of atomic hydrogen

and its application to electric arc welding; the effects of gases on electrically heated filaments as applied to gas-filled tungsten lamps, and the laws of electron emission as applied to radio and other vacuum tubes.

Dr. Langmuir was graduated from the School of Mines of Columbia University in 1903. He continued his studies at the University of Göttingen, where he received the doctorate of philosophy. Returning to this country, he served the Stevens Institute of Technology, Hoboken, New Jersey, as instructor of chemistry from 1906 to 1909. Since then, he has been connected with the General Electric Research Laboratories.

SCIENTIFIC NOTES AND NEWS

DR. T. W. STANTON, geologist in charge of paleontology and stratigraphy in the U. S. Geological Survey, has been appointed chief geologist, succeeding Dr. W. C. Mendenhall, who has become director of the survey. Dr. John B. Reeside, geologist, has been placed in charge of paleontology and stratigraphy.

DR. GEORGE E. COGHILL, professor of comparative anatomy at the Wistar Institute, Philadelphia, has been nominated for president of the American Association of Anatomists and will be elected at the meeting to be held in New York on March 25 and 26.

THE Chancellor's Medal, the award of the University of Buffalo for public service, was conferred at the midwinter commencement on February 22 on Dr. Frank A. Hartman, professor of physiology, one of the discoverers of the cortin treatment for Addison's disease.

THE Langley Memorial Prize, which is usually awarded every three years for the best paper submitted by officers of the West African Medical Service, has been awarded by the London School of Hygiene and Tropical Medicine in equal shares for Dr. E. C. Smith's paper entitled "A Dermatological Atlas of Nigeria" and Dr. Hope Gill's paper on "Diagnostic Methods in Human Trypanosomiasis."

DR. DAVIDSON BLACK, professor of anatomy at Peking Union Medical College, has been elected a corresponding member of the Field Museum of Natural History.

A DINNER in honor of the seventy-fifth birthday of Mr. Otto P. Amend, past president of The Chemists' Club, New York, was held at the club on February 3.

DR. JULIUS WAGNER-JAUREGG, who retired three years ago as professor in the Medical School of the University of Vienna and as director of the Psychiatric Neurological Clinic, will celebrate his seventy-fifth birthday on March 8.

PROFESSOR LUDOLF VON KREHL, professor of internal medicine at the University of Heidelberg, known for his work on pathological physiology, celebrated his seventieth birthday on December 26.

PROFESSOR ANSON MARSTON, dean of engineering at the Iowa State College, and Dr. Charles F. Curtiss, dean of agriculture and director of the Agricultural Experiment Station, will retire at the close of the present college year, having reached the age of retirement established by the Iowa State Board of Education.

DR. ROSCOE RAYMOND HYDE has been promoted to be professor of immunology at the School of Hygiene and Public Health of the Johns Hopkins University to succeed the late Dr. Carroll G. Bull.

DR. M. X. SULLIVAN, until recently biochemist of the U. S. Public Health Service, has become director of the Chemo-Medical Research Institute of Georgetown University.

MR. JOHN C. PHILLIPS has been appointed to the staff of the Peabody Museum, Cambridge, filling the vacancy caused by the death of Augustus Hemenway. Messrs. George W. Harley and Philip A. Means have been appointed associates in anthropology.

DR. CLARENCE BEAMAN SMITH, chief of the Office of Cooperative Extension Work of the U. S. Department of Agriculture, who has been associated with the department's extension work since its inception, has been appointed assistant director. Under this arrangement Dr. Smith will, in addition to continuing as chief of the Office of Cooperative Extension Work, assist Dr. C. W. Warburton, director of extension work.

DR. VICTOR C. JACOBSEN, professor of pathology, Albany Medical College, has been awarded a grant of $1,000, by the Committee on Scientific Research of the American Medical Association, to aid in a study of transplantable mouse melanoma, particularly in elucidating the neurogenic theory of melanoma origin.

DR. EDWARD ORTON, JR., from 1894 to 1916 director of the department of ceramic engineering at the Ohio State University and from 1902 to 1915 dean of the College of Engineering, later engaged in manufacturing, died on February 10, at the age of sixty-eight years. Dr. Orton was state geologist of Ohio from 1899 to 1906 and was the founder of the American Ceramic Society.

DR. BENNET M. ALLEN, professor of zoology at the

University of California at Los Angeles, sailed for Naples on February 25. He plans to make a study of the research work being carried on in European laboratories.

PROFESSOR RALPH W. HUFFERD, of the chemistry department of DePauw University, is spending his sabbatical year in Europe. With the exception of six weeks spent in the study of microchemical analysis with Professor Hans Leib, at Graz, Austria, he is devoting the year to a study of the teaching of chemistry in the universities and high schools of Germany, Austria, France and England.

AT the Bureau of Economic Geology, Texas, Mr. W. S. Adkins recently returned from several months spent in Europe as the holder of a Guggenheim fellowship; Mr. F. B. Plummer is on leave of absence in order to give courses in petroleum production engineering at the University of Texas, and Mr. C. L. Baker is giving lectures at Northwestern University.

PROFESSOR GEORG TISCHLER, director of the Botanical Institute at Kiel, delivered some 30 lectures on karyology of plants at the Johns Hopkins University while in residence as the James Speyer visiting professor from November 1 to January 30. He gave during February invitation lectures at several midwest universities and at the University of California. During March he will visit the Desert Laboratory at Tucson, New Orleans and the east coast of Florida for botanical collecting. He will sail for Europe on March 30.

DR. CLARENCE COOK LITTLE, director of the Roscoe B. Jackson Memorial Laboratory at Bar Harbor, Maine, formerly president of the University of Michigan, will be the speaker at the commencement exercises of the University of New Hampshire.

A SERIES of eight lectures on "The Problem of Human Wholeness" is being given at the University of California by Dr. William E. Ritter, professor emeritus of zoology.

THE Gehrmann Lectures at the University of Illinois College of Medicine will be given by Dr. C. Macfie Campbell, medical director of the Boston Psychopathic Hospital, and professor of psychiatry in the Harvard Medical School, on March 15, 16 and 17. The topics of Dr. Campbell's lectures are "Schizophrenic Disorders," "Preoccupational and Environmental Problems of the Patient" and "Personal and Environmental Problems, and Resources Available for Treatment."

TRUSTEES of the Ropes Memorial, Salem, Massachusetts, announce the twentieth lecture course of six lectures to be given from March 9 to April 13, on "The Geographic Isolation and the Evolution of Plants," by Professor M. L. Fernald, of Harvard University.

MR. FRED M. HARPHAM, vice-president of the Goodyear Tire and Rubber Company, gave the third Aldred Lecture of the year at the Massachusetts Institute of Technology on February 26. He spoke on "Zeppelin Airships—their Performance and Possibilities." The lecture was followed by motion pictures showing the construction of the dock and *U. S. S. Akron.*

DR. CHRISTIAN A. RUCKMICK, professor of psychology at the University of Iowa, has completed a state-wide lecture tour under the auspices of the Graduate College. Each year a lecturer is chosen on the grounds of his "distinction as an investigator" to present materials of his research to colleges and universities of the state. The subject of Dr. Ruckmick's lecture was "An Experimental Approach to the Study of the Emotions."

PROFESSOR WILLIAM SEIFRIZ, of the University of Pennsylvania, gave a course of lectures at the Johns Hopkins University on the structure of protoplasm during the first semester, taking the place of Dr. Duncan S. Johnson, who was on leave for duty at the National Research Council at Washington.

THE first annual Sigma Xi day at the University of Rochester, on February 22, was arranged by the local chapter to acquaint the rest of the university and residents of the city with the methods and aims of scientific research and with problems of investigation being conducted by members and their associates. Ninety-five exhibits of current research projects from the scientific departments of the university were on display in the Museum of Natural History from 11 A. M. until 5 P. M., and the exhibitors gave talks explaining their work. In the morning, a lecture intended especially for children was given by Dr. Wallace O. Fenn, professor of physiology, on the subject: "Arms and Legs and How they Work." At night, the society held a formal dinner, followed by an address on "Photography in the Service of Astronomy," by Dr. C. E. K. Mees, research director of the Eastman Kodak Company. Dr. Rush Rhees, president of the university, presided at the evening lecture.

ACCORDING to *Nature* the ninety-first annual general meeting of the British Chemical Society will be held in Glasgow on March 18 under the presidency of Professor G. G. Henderson, regius professor of chemistry in the University of Glasgow, who will take as the subject for his address "The Publication of Chemical Literature." This will be the first official visit of the society to Scotland.

THE House Committee on Foreign Affairs, after hearing on February 24 the director of the U. S.

Geological Survey, Dr. W. C. Mendenhall, deferred action on the resolution to authorize an appropriation of $85,000 for the expenses of the sixteenth session of the International Geological Congress to be held in the United States in June, 1933. Dr. Mendenhall told the committee that there will be from 300 to 500 delegates from foreign countries, and that a program for excursions, to be financed privately, was planned.

THE library of the late Dr. Calvin H. Kauffman, professor of botany and director of the herbarium of the University of Michigan, has been presented to the university by Mrs. Kauffman. It contains 365 volumes and a collection of about six thousand reprints.

ENGLISH exchanges state that owing to the closing of Chillingham Castle, Northumberland, efforts are being made to keep together the herd of wild cattle which is believed to have roamed in the castle enclosure since the thirteenth century. The Zoological Society of London has undertaken to contribute £100 a year for seven years if a lease at £500 a year for that period is granted to three trustees nominated by the society. The remainder of the £500 a year is to be provided by public subscription. The trustees are Lord Grey of Fallodon, Mr. Hugh S. Gladstone and Sir Peter Chalmers Mitchell.

A NEW zoological park "The Provincial Sanctuary and Zoo of Vancouver," is to be established at Vancouver, British Columbia. It is to be entirely for North American mammals and birds and is laid out through natural pine forests connecting two artificial lakes. Enclosures for the exhibits will be placed along trails in the woods. The plan of the zoo was prepared by Mr. C. Emerson Brown, director of the Philadelphia Zoological Garden.

THE Rockefeller Foundation recently granted $250,000 to Vanderbilt University School of Medicine for a research fund, with the understanding that a supplementary amount shall be secured by the authorities of the school. An appropriation of $50,000 has also been made by the General Education Board for the medical library. The research fund will be divided and distributed over a period of eight years, while the medical library grant will be distributed over a period of four years.

THE New York Hospital's new medical center, covering three square blocks along the East River between Sixty-eighth and Seventy-first Streets, is to be opened on September 1. New quarters will be provided for the Cornell University Medical College and a new psychiatric hospital and a new children's hospital will be added to the city's medical facilities. The hospital will have a potential capacity of 1,000. More than half the beds will be available for occupancy at the outset. Equipment and space for medical teaching and special research in all the major fields of medicine will be provided. The Cornell University Medical College will begin to move as soon as its present term ends next June. The college will occupy the buildings joined to those of the New York Hospital on the York Avenue side of the center, between Sixty-eighth and Seventieth Streets. Its fall term will open there on September 29. In its main building, which faces Sixty-eighth Street and rises twenty-six stories to a height of approximately 400 feet above the river, the New York Hospital will devote the two south wings to the public pavilions for medicine and surgery. The north wings will be occupied by the out-patient department and special therapies. The ninth floor will be given over to the James Buchanan Brady Urological Clinic. Private patients are to have a self-contained hospital which will occupy the lower section of the tower, the twelfth to the seventeenth floor. The upper floors are to be used as quarters for the resident medical, surgical and executive staffs.

THE Paris correspondent of *The British Medical Journal* writes: "The congresses are over, and every department of medicine has been discussed. France should now be able to take good care of her population, but, as it was forcibly pointed out at the Congress of Social Hygiene held at Mulhouse, the first necessity is that there should be a population. Exclusive of foreigners the population of France is about 38 millions, roughly the same as it was just before the war of 1870. The population of Germany over this period, however, has increased from 39 to 65 millions, and that of Italy from 25 to 42 millions. The birth rate of France has decreased by more than 30 per cent., and it is probable that it will go on decreasing; this is partly because of the comparatively small number of births during the war, and partly because the younger generation seems strongly in favor of birth control. A declining population is the greatest danger we have to face. The medical profession is doing its utmost to fight infant mortality, and Dr. Devraigne, well known as an ardent protector of both mother and child, showed us a film, which is to travel throughout France, illustrating the principles of hygiene—the care of the child, hygiene in the home, the consequences of alcoholism, protection against infectious diseases, etc. The congress dispersed after having proposed that the government should consider all questions connected with ante- and post-natal care of the mother and child as of the first importance, and, further, that taxation should vary in accordance with the number of children in the family."

Industrial and Engineering Chemistry writes: "The Chemical Laboratory of the American Medical Asso-

ciation, established in 1906, celebrated its twenty-fifth anniversary in October, having functioned continuously for a quarter of a century. The primary reason for its establishment was that the Council on Pharmacy found it difficult to secure satisfactory outside help in checking the composition and properties of newer drugs under investigation and in watching over market supplies of non-official preparations. Since that time the work of the laboratory has been greatly extended, it has aided the medical profession in taking a much more scientific attitude toward drugs, and has rationalized prescribing. It has also been the means of informing the medical profession whether compounds are of the chemical composition claimed. Gross deceit has lessened, though subtle forms of fraud are still practiced. It has been due in great part to the work of the laboratory that the boards of directors of progressive pharmaceutical firms have seen the necessity of maintaining high-grade scientific staffs in order that their products may be carefully watched and freed from criticism and that research may be along lines of genuine value. The laboratory has had a large part to play in scientific nomenclature, in controlling scientific names which twenty-five years ago were in a chaotic condition, and has done its part in developing better methods of standardization and maintaining the best chemical technic and procedure. It has been an aid to public health, in that certain new developments which could not meet its tests have gone no farther than the laboratory."

DISCUSSION

THE GLACIAL CONTROL THEORY APPLIED TO BERMUDA

OUT in the Atlantic Ocean, 675 nautical miles southeast of New York City, stands the submerged volcano making the limestone-covered islands of Bermuda, with their winter resorts, their international biological station, and their naval base of the far-flung British Empire. When this old volcano was active, and when its fires died out, is not known, but presumably at some time in the late Cenozoic. To these beautiful islands Dr. Robert W. Sayles, of the Harvard Department of Geology, went on a vacation trip during the winter of 1923, and in his walks he noted at many places the more or less red residual soils interbedded in the wind-blown dune limestones, now more or less consolidated. Then and there the thought came to him that in these soils, with their living and extinct land snails, might lie the means of correlating these strata with the various epochs of the Pleistocene glacial history of North America. Since then he has revisited Bermuda several times, and now we have his matured considerations, set forth in an interesting memoir[1] that shows much originality and industry. It blazes a new line of endeavor in testing the glacial control theory of Daly, and the method should next be applied to the Bahamas.

The residual soils, Sayles discovered, occur throughout the islands, and they contain 24 described forms of land snails, of which at least 6 and possibly 11 are extinct. These snails furnish the chief means of determining the age of the soils. Probably not enough detailed collecting and study was done to prove beyond a doubt the time correlations. However this may be, many of the local soils are arranged into five named horizons that are separated from one another by the dune limestones, and are correlated with the warm intermediate times between the Wisconsin, Illinoian and Yarmouth epochs of the glacial record. The underlying, much consolidated Walsingham limestone, also largely of dune origin, is correlated with the Kansan and older divisions of the Pleistocene. The underlying basis of interpretation is Daly's Glacial Control Theory, which holds that great quantities of water were subtracted from the oceans to form the continental ice-sheets:

> When the ice was at its maximum extent the strandline fell as much as 260 feet below modern sea-level. While the ice-cap grew, large parts of the Bermuda banks, covered by mollusc shells and unprotected by vegetation, were exposed to the sweep of the winds and the dried sands were piled up in great dunes. . . . When the sea rose at the close of each glacial stage, the source of supply for the dunes was buried beneath the ocean waves, the winds became less violent, and a permanent flora anchored the dunes. A long period of slow decay began, during which red and brown soils accumulated [p. 460].

Accordingly, the Bermudas are considered to have stood highest above sea-level, with the stormiest climate, during the glacial times, causing the formation of the dunes, now consolidated into eolianites, a term here proposed "for all consolidated sedimentary rocks which have been deposited by the wind." During the interglacial warmer times, the sea-level rose and the islands were reduced in size to about their present extent, and this is when the residual soils were formed and the snails entombed.

CHARLES SCHUCHERT
YALE UNIVERSITY

[1] "Bermuda during the Ice Age." By Robert W. Sayles. Proc. Amer. Acad. Arts Sci., Vol. 66, No. 11, November, 1931, pp. 379–467, pls. 1–13, text figs. 1–18.

COBALT IN PLANT ASH

IT was observed when vegetables were being ashed for calcium determinations that some gave a white

to brown ash, others a green or blue ash. The latter produced a green or blue water solution which turned pink on the addition of hydrochloric, nitric or sulfuric acid. The hydrochloric acid solution soon faded, but the nitric and sulfuric acid solutions retained their color for twenty-four hours.

The tendency to form the green or blue ash depends on both the soil and the vegetable. Of the plants grown on the Alabama Experiment Station at Auburn, New Zealand spinach and chard gave an intense green ash; tendergreen and Chinese cabbage gave a medium green ash; while the ash from turnips varied from green in some cases to brown in other cases. Cabbage grown in the greenhouse on Norfolk, Cecil and Hartselle soils gave a green ash of varying intensities; that grown on Eutaw soil gave a white ash. Turnips similarly grown on Oktibbeha soil gave a pale green ash.

Since the Allison apparatus[1] gives one or more light minima characteristic of each compound present and since it will detect approximately four parts in 10^{12}, it seemed a desirable means to determine the cause of the green color referred to above. Several samples of vegetables were ashed, dissolved with hydrochloric acid and examined for the chlorides of iron, chromium, manganese, cobalt, nickel and copper. Cobalt was always absent in the white or brown ash but always present in the green or blue ash, nickel was always absent and the other elements listed above were always present. Hence, it is concluded that the green or blue ash is due to the presence of cobalt. No relation was observed between plant growth and the presence or absence of cobalt in the ash.

We wish to express our appreciation to Dr. Allison for the use of his apparatus and his interest in the work.

EDNA R. BISHOP
MARGARET LAWRENZ

ALABAMA EXPERIMENT STATION,
AUBURN, ALABAMA,
NOVEMBER 25, 1931

THE RELATIONSHIP OF THE GRANITES TO THE RHYOLITES IN SOUTHEASTERN MISSOURI

HAWORTH,[1] in his account of the igneous rocks of Missouri, states that the granites, with few exceptions, grade into the porphyry (rhyolite). He cites many localities where such gradation can be seen and draws the following conclusion. "It is therefore useless to attempt to decide which is the older, the granite or the porphyry." He emphasizes the fact that the granite occurs in the lower ground between the higher hills of porphyry and concludes that the porphyry represents the more rapidly cooled portion at the surface, and the granites and granite porphyries the slower cooled portion deeper down, and that all the rocks belong to the same eruption of igneous material and hence grade one into the other.

This relationship, as interpreted by Haworth, has been accepted by the present Missouri Bureau of Geology and Mines, as is shown by the following quotation from its report on the "Quarrying Industry of Missouri" (p. 61). "The gradation of rhyolite into granite and *vice versa* can often be traced horizontally and vertically."

For many years, I have doubted that a granite in which the grains average nearly one half inch in diameter (which is true of some of those occurring in southeastern Missouri) could change to a dense rhyolite showing flow structure within a few feet, as, under the Haworth gradational theory, would be required by the relationship of many occurrences of these two rocks. It was not until 1919, however, that I found the first positive proof that the granite was instrusive into the rhyolite. This locality was near the top of Knoblick Mountain in St. Francis County, which is in the northeastern part of the area of igneous rocks. The contact found was so sharp and clear-cut that there was absolutely no doubt that the granite was later than the rhyolite and that it was intrusive.

During subsequent years, I have searched for further evidence on the problem, and have been rewarded from time to time by finding other localities where similar relationships exist, although the widespread mantle rock has handicapped the search for actual contacts.

During the last three years, an intensive search throughout the area of igneous rocks of southeastern Missouri has resulted in finding evidence that the granites are not only younger than the rhyolites, but that they were injected into them. Sharp contacts of medium to coarse-grained granites with the dense rhyolites that were undoubtedly surface flows have been found in every locality where the relationship of the two rocks has been determined. This intensive study has shown that probably more than one granite injection occurred, or else that there was marked differentiation within a large intrusive; more probably the former. A detailed account of the relationship of these two rocks was given at the meeting of the Geological Society of America in December, 1931.

W. A. TARR

DEPARTMENT OF GEOLOGY,
UNIVERSITY OF MISSOURI

[1] Allison and Murphy, *J. Am. Chem. Soc.*, 52, 3796, 1930.

[1] E. Haworth, "The Crystalline Rocks of Missouri," Mo. Geol. Surv., Vol. 8, 84–220, 1895.

THE RÔLE OF CANNIBALISM IN THE SPREAD OF FUNGOUS DISEASES OF GRASSHOPPERS

FUNGI of the genera Entomophthora and Empusa have long been known fatally to infest Melanoplus and other genera of grasshoppers. The body of the host remains clinging to the stalk where it died and transmits the disease by allowing the spores of the parasite to come in contact with healthy individuals, and, under favorable conditions, to germinate in their bodies.

Uninfected grasshoppers may themselves aid in the spread of the disease by virtue of a cannibalistic tendency present in certain species. As a demonstration of this tendency, freshly killed individuals of *Melanoplus bivittatus* "planted" along an open path served as bait to attract other individuals of the same species. The cannibals, after making an incision through the first abdominal tergites, would greedily devour the softer substances of the interior of the thorax, leaving the abdominal organs, however, untouched.

Observations made on individuals of this species that had died as a result of parasitism by Entomophthora and Empusa reveal that, although most of them had been untouched by their cospecific cannibals —probably because of the slower rate of dying, or because the bodily odors are more suppressed than in the artificially killed individuals—yet in a few cases the thorax had been hollowed out in precisely the same manner as in the "planted" specimens; and in one instance the act of cannibalism was itself witnessed by the writer. If mere contact, or the chance settling of an Entomophthora or Empusa spore on Melanoplus can bring about infection under the proper conditions (as it unquestionably can), this intimate contact ought all the more to be able to do so; and the moisture transmitted by the dead insect's body to that of the prospective host should make germination of the spores, and the consequent infection, practically certain.

MAURICE T. JAMES
UNIVERSITY OF COLORADO

THE IRON-DEFICIENCY HYPOTHESIS IN PELLAGRA

IT is desired to correct some misstatements that appeared in an article written by Nellie Halliday.[1] Her reference to an article by Bliss[2] accepts more for his views than he had any intention of claiming, and is, therefore, misleading. Bliss, far from "claiming to show that pellagra . . . and black tongue . . . are both due to an iron deficiency," stated that "none of the considerations here outlined prove that pellagra is an iron deficiency disease," and asserted that he "has adopted the working hypothesis that pellagra is an iron deficiency disease, and has set out to *prove* or *disprove the thesis.*"

One point which Miss Halliday appears to have tried to make is in connection with the probable value of inorganic iron salts in curing the bilaterally symmetrical lesions occurring in rats on a low-iron diet. The failure of $FeCl_3$ in this respect is entirely to be expected, and follows from the results published by Bliss and Thomason,[3] which may have escaped the attention of Miss Halliday. Bliss and Thomason stated that "the addition of ferric citrate to the drinking water or the addition of solid ferric citrate to the food has no demonstrable effect upon the symptoms over a period of months." This finding has been confirmed and reported by others.[4]

Familiarity with living conditions in the south would probably have prevented Miss Halliday from calling attention to the iron content of commercial molasses as an apparent discrepancy in the iron hypothesis. Farmers of the South do not use the concentrated molasses preparations in question, but syrup prepared upon their own farms. Analyses of products subjected to commercial plant manipulations throw no light on the question in hand. Pellagra in the South is essentially a rural problem.

The efficacy of inorganic iron preparations given by mouth has little to do with a decision for or against the iron hypothesis. Work in this laboratory indicates more clearly than before that in pellagra we are dealing with an iron deficiency, but we are refraining from publishing fragmentary announcements until the evidence from many angles can be assembled and evaluated.

For the present we are only desirous that any attempts to refute the iron hypothesis, whether they come from our own laboratory or another, be based upon sound work of a comprehensive character.

SIDNEY BLISS
SCHOOL OF MEDICINE,
TULANE UNIVERSITY

[1] N. Halliday, "The Lack of Correlation between Anemia and the Pellagra-like Symptoms in Rats," SCIENCE, 74: 312, 1931.

[2] S. Bliss, "Considerations Leading to the View that Pellagra is an Iron-Deficiency Disease," SCIENCE, 72: 577, 1930.

[3] S. Bliss and M. L. Thomason, "Iron Deficiency in the White Rat and the White Mouse," *Proc. Soc. Exp. Biol. and Med.*, 28: 636, 1931.

[4] B. Sure, M. C. Kik, M. E. Smith, "Hematopoietic Function in Avitaminosis. VI., Vitamin G Deficiency," *Proc. Soc. Exp. Biol. and Med.*, 28: 498, 1931.

KREEZER AND DARGE ON AUDITORY ACTION CURRENTS

G. KREEZER and H. DARGE, in the January 22 issue of this journal, have reported their inability to reproduce the results of our auditory nerve experiments. And they suggest the "possibility" that we have neglected one of the more elementary precautions required in an experiment of the kind.

It should be unnecessary to state that this precaution was taken, or to inform Kreezer and Darge that our experiments have been repeated and our results reproduced by Adrian (*J. Physiol.*, 1931, *71*, pp. 28–9 *P*), Adrian, Bronk and Phillips (*ibid.*, 1931, *73*, pp. 2–3 *P*), Davis and Saul (SCIENCE, 1931, *74*, 205–6), Rademaker and Bergansius (*Arch. néerl. de physiol.*, 1931, *16*, 346–9), and Crowe and Hughson (*Zsch. f. Hals-, Nasen- u. Ohrenheilk.*, 1931, *30*, 65–76; *J. Amer. Med. Assoc.*, 1931, *96*, 2027–8).

E. G. WEVER
C. W. BRAY

PRINCETON UNIVERSITY

SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE CENTRIFUGE-MICROSCOPE FOR SUPER-CENTRIFUGAL FORCES

IN 1930, Harvey and Loomis[1] described a form of centrifuge-microscope for observing living material while being centrifuged. It consists of a special centrifuge head containing a microscope objective mounted at the circumference under the special slide of living cells. Right angle prisms bring the image to the axis and then vertically upward, where it is observed by an ocular which does not rotate. A high voltage discharge in mercury vapor illuminates the material synchronously with each revolution of the microscope. Although the image is perfect, even at highest powers, the size of the head (2⅝ inches thick, resisting air) offers so much air resistance that speeds over 4,000 r.p.m. (about 2,000 times g at 11 cm radius) can not easily be attained. In recent models the air resistance has been greatly diminished by using objective lenses only and small right angle prisms mounted either between objective and object or between the lenses of the objective system. In this way the thickness of the head has been reduced to ½ inch, and by stream-lining the contours, it is possible to run up to 10,000 r.p.m., giving forces of 12,000 times gravity. This type of head is most suitable for general biological work.[2]

However, it is possible to adapt the microscope-centrifuge principles to the Beams[3] ultra-centrifuge, by which forces approaching one million times gravity can be attained. This scheme is the simplest possible arrangement and has worked out remarkably well, in fact far beyond expectation. No lenses but only mirrors revolve. As shown in Fig. 1 two stellite mirrors

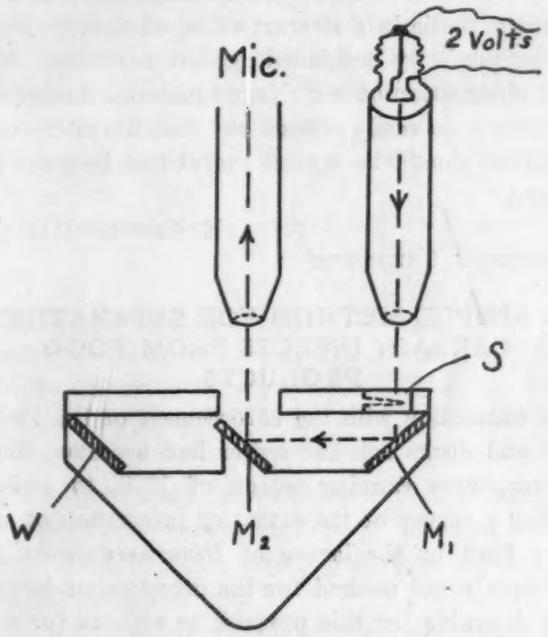

FIG. 1

(M_1 and M_2) are mounted on the Beams' rotor in such a position that the image of the object on a special slide (S) is brought to the axis and reflected into a microscope (Mic) mounted above and on the axis of the rotor. W. is a counter weight for balancing the rotor. The illumination is a narrow image of the filament of a straight filament tungsten lamp thrown on the material to be observed parallel to a radius of rotation. A relatively large movement at the circumference becomes a very small movement when the image is brought to and observed on the axis of rotation. While the whole field of view is not perfect, the center is good enough for all practical purposes. The magnification of this scheme is limited by the working distance of the objectives; ×5 to ×7 objectives can be used giving with ×20 oculars, 100 to 140 diameters. By mounting lenses on the rotor higher

[1] E. N. Harvey and A. L. Loomis, SCIENCE, 72, 42, 1930.

[2] The high speed heads were made at the Alfred L. Loomis Laboratory at Tuxedo Park, N. Y. I express my sincere thanks to Mr. Loomis for his generous hospitality and advice during the progress of the work. I am indebted to the Bausch and Lomb Optical Company for lenses and prisms used in developing the new form of head. This company expects to place the microscope-centrifuge on the market in the near future.

[3] J. W. Beams, *Rev. Sci. Inst.*, 1, 667, 1930: SCIENCE, 74, 44, 1931. Dr. Beams and Mr. Weed, of the University of Virginia, have constructed one of the rotors with stellite mirrors which works perfectly.

magnification can, of course, be used. The centrifugal force attainable by means of the Beams' rotor is limited only by strength of materials, and for microscopic observations by this method, is determined by the strength of the glass container of the living cells. This might be put at 200,000 times gravity. Such an arrangement should be particularly useful for determining molecular weights of substances by the method of sedimentation, for observing movement of materials in highly viscous cells and for observing the change in shape of living cells due to the stretching forces of light and heavier material. From such observations one is frequently able to gain an idea of the surface and other forces which counteract distortion.[4] In order that a cell may not be completely crushed by forces thousands of times gravity, it is necessary to adopt the expedient of suspending the material in a medium of graded density, so that the cell comes to lie in a stratum of equal density, and is thereby perfectly cushioned against crushing. Magnified observation of centrifuged material during centrifuging is so easily carried out that the microscope-centrifuge should be a most useful tool in every laboratory.

E. NEWTON HARVEY

PRINCETON UNIVERSITY

A SIMPLE METHOD FOR SEPARATING CERTAIN INSECTS FROM FOOD PRODUCTS

IN connection with the enforcement of the Federal food and drugs act, the writer had occasion, during the raspberry canning season of 1929, to assist in making a survey of the extent of infestation of raspberry fruit by the larvae of *Byturus unicolor* Say. A simple rapid method for the detection of larvae is very desirable for this purpose as well as for determining the fitness of the fruit for food purposes prior to canning. A description of the method as devised is given at this time in the thought that it may be of general use in entomological studies.

Methods of decantation with water were found to be inadequate because the large number of seeds in the raspberries interfered with the rapid detection of the larvae. After considerable experimentation it was found that the incorporation of some oily substance, such as kerosene or gasoline, with the cooked and pulped raspberries caused the larvae to rise to the surface of the subsequently diluted liquid. It was originally thought that the larvae must first be saturated with the oil, and for that reason a method of shaking the oil with a partially diluted pulp in a large Mason jar was devised. While this method gave good results, it was soon found that prolonged shaking was unnecessary, it being essential merely to bring the larvae into contact with the oil-water interface.

The method as recommended for factory testing of raspberries is given below. For other purposes changes will no doubt be advisable. The esssential points in detecting the larvae by the use of gasoline or kerosene are: (1) Maceration of the fruit, (2) formation of oil globules throughout the liquid, and (3) careful agitation at end of test to permit all the larvae to come to the top.

The materials required are: (1) Gasoline or kerosene, (2) two No. 10 cans (approximately 3,100 cc each), (3) one No. 2 can (approximately 580 cc), (4) a large spoon or paddle, (5) source of heat, (6) small graduate, (7) forceps and (8) a shallow pan.

PROCEDURE

(1) Fill the No. 2 can with the raspberries to be tested.

(2) Pour berries into a No. 10 can, crush with spoon and cook until soft. If steam heat is used see to it that it is as dry as possible so as not to allow the can to become more than one fourth full of pulp and steam condensate. With dry heat, add enough water to facilitate cooking.

(3) Add 1 to 2 ounces of gasoline or kerosene to the can and stir vigorously.

(Caution: The need of guarding against fire and explosion should not be overlooked.)

(4) Fill the can nearly full with water.

(5) Allow water and pulp to come to rest and examine the surface for larvae. All insects should be picked off with the forceps as soon as located.

(6) Since some of the larvae may be entangled in the pulp, it should be stirred several times, and the surface examined after each stirring. In stirring, the water should be given a rotary movement, then, by placing the spoon in a slanting position along the side of the can, an upward current can be established which helps to bring the larvae to the surface.

Some prefer to pour the contents of the No. 10 can directly into a shallow pan, rinsing out the No. 10 can into the pan and adding enough water to fill the pan to the depth of about 2 inches with liquid and pulp.

(7) As a final precaution, pour the mixture from one No. 10 can to the other, or from pan to can, once or twice, and examine the surface each time for insects.

Insects commonly found in infested foods, in general, respond to the gasoline treatment. This method, or some modification of it, has been found useful in the examination of cereal products, fig paste, spinach and turnip greens, as well as raspberries. Unfortunately the maggot of the cherry fly and of the blueberry fly, and possibly other maggots, do not rise to the surface on such treatment.

[4] E. N. Harvey, *Biol. Bull.*, 60: 67, 1931; 61: 273, 1931.

Trials of the method have been made during the 1929, 1930 and 1931 seasons by the simple expedient of adding a known number of live larvae to the sample before testing. In only two instances out of 76 trials was there a failure to recover the full number of larvae added.

J. D. WILDMAN
MICROANALYTICAL LABORATORY,
FOOD AND DRUG ADMINISTRATION,
U. S. DEPARTMENT OF AGRICULTURE

SPECIAL ARTICLES

PATERNAL AND MATERNAL INHERITANCE IN FRAGARIA

IRREGULARITIES in the behavior of Fragaria in breeding have been recorded by several investigators. Many cases are recorded where the F_1 seedlings have the characteristics of the female parent only, while a few cases are mentioned by Millardet[1], Solms-Laubach[2], Longley[3] and Ichijima[4], where the F_1 seedlings have the characteristics of the male parent only. In all the cases where pure maternal or paternal inheritance has been observed by the various writers, the seedlings have been crosses between parents having different chromosome numbers. That this type of inheritance also occurs in cases where two plants with the same number of chromosomes are crossed is shown by results obtained at the Oregon State Experiment Station at Corvallis, Oregon.

Several different selections of the wild field strawberry, commonly known as *Fragaria cuneifolia,* were crossed with horticultural varieties by C. E. Schuster in 1929. These were set in the field in March, 1930, and the results were observed in May and June, 1931. One set of crosses consisted of the horticultural variety Gold Dollar[5] (derived from *F. virginiana* x *F. chiloensis*) x *F. cuneifolia,* both of which have 28 chromosomes. In a set of 2,015 seedlings there were 37 plants which showed paternal inheritance only. In a set of 420 plants of Ettersburg 121 x *F. cuneifolia* there was one plant that showed apparently pure paternal inheritance and three that were doubtful. A set of 948 plants of the cross U. S. D. A. No. 147A x *F. cuneifolia* contained two plants with apparently pure paternal inheritance and seven dwarfs. In all there were 43 plants with paternal inheritance only out of 3,519, or 1.2 per cent.

In addition to the above crosses, using *F. cuneifolia* as the male parent, there were two sets of crosses in which pistillate selections of this species were used as the female parents. In a set of 207 plants of the cross *F. cuneifolia* x Gold Dollar, there was one plant with apparently pure maternal inheritance. The flowers were pistillate but sterile. Another selection of *F. cuneifolia,* also crossed with Gold Dollar, contained one plant which appeared to be pure paternal in inheritance in a set of 216 plants. There was also a set of 216 plants of the cross Corvallis x *F. cuneifolia* which had no cases of pure maternal or paternal inheritance that could be detected.

The plants which showed the inheritance from the species (*F. cuneifolia*) parent only were much smaller than the true hybrids, and had all the plant characters of the wild species. A single typical plant of each of the two types in the set of Marshall x *F. cuneifolia* crosses was measured with the following results:

	Plant with pure paternal inheritance	True cross
Fresh weight in grams	130	700
Number of leaves	67	105
Average size of leaves, sq. cm.	16.6	106
Total leaf area, sq. cm.	1,109	11,130

These figures are from only one plant of each kind, but they were representative of the lots and the figures are sufficiently striking to show the difference between the types.

The true hybrids were large vigorous plants, showing the effect of hybrid vigor. Almost without exception they were larger than most of the crosses between cultivated varieties. One plant of the cross Ettersburg 121 x *F. cuneifolia* had as many as 1,352 flowers on it. They showed considerable variation, but were intermediate in most characters between the types of the two parents. The plants with the pure paternal or maternal inheritance of the species were almost or entirely sterile, but the berries produced were distinctly of the type of the species.

No chromosome counts have been made on these plants, and it is not known whether the haploid or diploid number prevails in the somatic tissue of the plants showing inheritance from only one parent. If only one nucleus went into the make-up of the plants the number could have been reduced to haploid through the lack of conjugation, or the diploid number may have been restored by doubling in microsporocyte, as suggested by Ichijima.

It is easily possible that this type of inheritance

[1] A. Millardet, "Note sur hybridation sans croisement ou fausse hybridation." *Mem. Sci. Phys. et Nat. Bordeaux,* 1894, 4 ser., IV: 347–372.

[2] H. Solms-Laubach, "Uber unsere Erdbeeren und ihre Geschichte." *Bot. Ztg.* 65, 1 Abt. 45–76, 1907.

[3] A. E. Longley, "Chromosomes and Their Significance in Strawberry Classification," *Jour. Agr. Res.,* 32: 559–568, 1926.

[4] K. Ichijima, "Studies on the Genetics of Fragaria," *Zeitschrift für Induktiv Abstammungs und Vererbungslehre,* Bd. L V, Heft 4: 300–347, 1930.

[5] In all references to strawberry crosses the female parent is mentioned first.

is common in the crossing of strawberry varieties but that it has not been recognized because of the similarity of the parent plants used. In the cases reported here, the parent plants were so strikingly different in size and appearance, in spite of their having the same number of chromosomes, that paternal and maternal inheritance could be easily recognized. The knowledge that this type of inheritance can be obtained from crossing two plants of the same chromosome number which readily hybridize is useful in making studies on inheritance, and it will obviate the necessity of working with crosses between plants of different chromosome numbers which are rather difficult to obtain.[6]

G. LEONARD RYGG
OREGON STATE AGRICULTURAL COLLEGE
AND U. S. DEPARTMENT OF AGRICULTURE
GEORGE M. DARROW
U. S. DEPARTMENT OF AGRICULTURE

PASSIVE IMMUNITY TO INFECTION WITH A LARVAL TAPEWORM OF THE ALBINO RAT[1]

THE results of a preliminary experiment show that the albino rat can be protected against infection with onchospheres of a cat tapeworm, *Taenia taeniaeformis*, by transfer of serum from immunized rats. This appears to be the first demonstration of transfer of what are probably circulating antibodies resulting from intra-peritoneal injections of cestode material.

It has been shown[2] that an active acquired immunity against infection by onchospheres of *T. taeniaeformis* can be artificially produced in the albino rat as a result of a standard series of six intra-peritoneal injections of a 1 per cent. suspension of powdered worm material of the same species. Complete or almost complete inhibition of development of *Cysticerous fasciolaris* (larval stage of *T. taeniaeformis*) in the liver of the rat resulted from the injections; while cyst development took place normally, for the most part, in control animals. Such artificially immunized rats were used in the present experiment as a source of the serum which was donated to the experimental rats. Thirty-four rats were given the standard series of injections from July 24 to August 8, 1931; they were reinjected with 2 cc of a 1 per cent. suspension of worm material on October 5 and 7. Serum was collected on October 9 and used the following day; normal serum from stock rats was also obtained on October 9.

Fifty-nine rats from 6 litters born May 16 to June 1, 1931, were used; individuals of each litter were distributed through three groups: one (Group A, 22 rats) to receive immune serum; another (Group B, 12 rats), normal serum; and the third (Group C, 25 rats) untreated. All rats were infected with equal portions of a uniform suspension of onchospheres, and the injection of serum begun 2 hours later. The rats of Group A received either 2.5 cc or 7 cc of immune serum intra-peritoneally, and those of Group B either 3 cc or 7 cc of normal serum. All animals were autopsied 34 to 38 days after infection. The data are shown in the following table.

Further experiments are in progress.

	Treatment	Number of rats	Average number of cysts	
			Living	Dead
Group A	7 cc immune serum	10[3]	11	25
		2	142	8
Group A	2.5 cc immune serum	3	12	46
		7	110	19
Group B	3 cc or 7 cc normal serum controls	12	167	14
Group C	Uninjected controls	25	276	16

HARRY M. MILLER, JR.
MARGARET L. GARDINER
DEPARTMENT OF ZOOLOGY,
WASHINGTON UNIVERSITY,
ST. LOUIS

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[6] Published as Technical Paper No. 157, with the approval of the director of the Oregon Agricultural Experiment Station, a contribution of the department of horticulture.

[1] This investigation was in large part made possible by a research grant made to Washington University by the Rockefeller Foundation.

[2] H. M. Miller, Jr., *Proc. Soc. Exp. Biol. and Med.*, 27, 926, 1930; *Jour. Prev. Med.*, 5, 429, 1931.

[3] No living cysts present in four rats.